DOCUMENT RESUME

ED 228 841 FL 013 595

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TITLE Problems in the Comparative Analysis of Bilingual Education.
PUB DATE Sep 82
NOTE 16p.
PUB TYPE Viewpoints (120)

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS *Bilingual Education Programs; Bilingualism; *Case Studies; *Comparative Analysis; Cross Cultural Studies; Educational Research; Language Research; Minority Groups; *Research Methodology; Testing

ABSTRACT

A review of research reveals little attention paid to the problems of a comparative approach to the analysis of bilingual education (BE). This paper explores some of the functions and problems of a comparative study of BE to clarify the question of whether BE research can be generalized. While problems of comparative research are common to all social and behavioral research, in BE research there is the frequent phenomenon of contradictory data. This has led to the need for cross societal, cross cultural, and cross national approaches. In order to identify the conditions under which children will master two languages, a case study approach is called for. This approach, although an appropriate one, brings with it the problem of comparability of variables, such as the comparison of programs for migrant children with Canadian immersion programs. Other difficulties are the problems of sampling, finding indicators that are unique to research on bilingualism, the matter of quantitative and qualitative data, and the problem of testing. In addition to these methodological problems, there are also theoretical, analytical, and ethical problems. It is hoped that this review of problems will contribute to researchers' good judgment in understanding the problems specific to BE research. (AMH)

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September, 1982

PROBLEMS IN THE COMPARATIVE ANALYSIS OF BILINGUAL EDUCATION

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ED228841

Virtually all the research on multilingual bilingual education (BE) is comparative in nature, yet we have not really paid any attention to the problems which a comparative approach entails. The only reference in the literature on bilingual and migrant education which I have come across which specifically mentions some of the problems of a comparative analysis is Ekstrand's "Migrant Adaptation: A Cross-Cultural Problem" (1978:30) and then only in passing. In this paper I would like to explore some of the functions and problems of a comparative study of bilingual education in order to clarify the question of generalizability of BE research.

As Simon points out"(m)ost empirical research in psychology, sociology, marketing research, education, anthropology, political science, and all other branches of social science except economics is comparison research, although sometimes the comparison is part of research intended to establish cause and effect. (1969:63)." Comparative study takes different forms in the various disciplines, basically because of the different problem formulations. One claim is that the term "comparative approach does not, as has sometimes been claimed, properly designate a specific method in social research, but rather a special focus on cross-societal, institutional, or macrosocietal aspects of societies and social analysis" (Shils in Eisenstadt 1969:423). The methodological problems then are not distinct

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from those of any other type of sociological research except as the choice of topic in comparative study may necessitate special types of data.

In sociology, comparative research usually deals with hypothesis testing about social behavior and institutions, through statistical techniques, over a wide sample of societies in order to find universal trends and general laws. Social anthropologists (in which field the term "comparative method" first seems to have become established) similarly work for "the development of general propositions about culturally regulated human behavior" which some believe will lead to the discovery of true sociological "laws", similar to the law of gravity. Leach (1969:339) points out that this analogy with the natural sciences simply does not work in the study of man because in contradistinction to the subject matter of natural science, man has a will of his own. Instead of discovering laws, the purpose of cross-cultural comparison in social anthropology should be "to discover what is humanly feasible rather than to demonstrate what is statistically probable. Cross-cultural comparison here becomes a means of understanding the humanity of human beings. It is not a question of demonstrating that culture is like nature, but of showing how culture differs from nature" (341:342). In psychology,[1] comparison research need not be cross-cultural but consists of what research is commonly held to be: experimental design andhypothesis testing with pre-specified variables through

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[1]Comparative psychology proper is concerned with the behavior of different species of living organisms, typically animals, and leads to the specification of similarities and differences in behavior between species in order to relate an animals behavior to its evolutionary background. (Waters and Bunnel 1969).
"Cross-cultural psychology is a meta-method with all of the areas of psychology grist to its mill". (Brislin et. al. 1975:7)

the collection of quantificational data from groups which are then compared. The problems of comparison here are common to all social and behavioral research but when this type of research design attempts to elucidate questions of bilingual education, there are problems associated with the specific types of data. For example, the crucial necessity to control for SES in studies on BE programs is (or should be) common to all research, but Cummins' attempt of theory building of L_1 and L_2 acquisition and their interdependence necessitates the distinction in the language data between CALP (cognitive-academic language proficiency) and BICS (basic interpersonal communicative skills), a problem of conceptualization and operationalization of data specific to this problem formulation. (Cummins 1982) Bilingual education, certainly not a discipline of its own, draws primarily on psychology, anthropology, and sociology in its research and so will share the functions and problems of research in those fields. Science does not solve problems, but it can help people make better decisions. That I and so many other people have stopped smoking is certainly the direct result of the findings of medical research, to take an example which involves individual decision making. In bilingual education, the research is typically marked by a practical and empirical approach, often specifically designed to help people make better decisions, such as Löfgren's Modersmålsklasser eller sammansatta klasser för barn till invandrare (1981) ('Mother tongue classes or integrated classes for children of immigrants: A theoretical discussion and empirical testing of some selected propositions and development of a casual model for immigrant children's success in school,' my translation) or Wrede's Elevers, föräldrars och lärares uppfattning av arbetet i tre finska lågstadieklasser i Eskilstuna kommun (1979) ('Students', parents' and teachers' experience and opinion of the

work in three Finnish elementary classes in Eskilstuna township,' my translation). The problem is of course that the decisions they advocate are diametrically opposed, and I think it is exactly the frequent phenomenon of contradictory data in the research of BE which has led to the necessity of a cross-societal, cross-national, cross-cultural approach in order to find universal trends and test hypotheses for the schooling of children in another language than their mother tongue. I have in mind such studies as Ekstrand's Migrant Adaptation: A Cross-Cultural Problem (1978) or my own Ethnic Relations and Bilingual Education: Accounting for contradictory data (1975).

Much BE research is evaluation research of specific programs and so by necessity treats the program as the independent variable, but I think most of us are by now in agreement that such findings hold only limited generalizability and that to understand such findings one has to consider school programs or treatments as intervening or contextual variables and look to socio-economic and cultural factors for causal explanations of language acquistion, of school grades, of social mobility, of employment rates, of however one chooses to operationalize program success. One of the major tasks, then, in the comparative study of BE becomes to identify under what conditions, the classical question of sociology, children will successfully manage schooling in two languages. This approach typically consists of an analysis of case studies (the evaluation/measurement case studies are not only useful here but indispensable) in light of some guiding hypotheses.

At times the theories which form the bases for studies are also examined in

a comparison of the case studies which serve to document the theories: such a study is Ekstrand's Early Bilingualism: Theories and Facts (1979) where his findings support Cummins' that older students make more efficient language learners. Cummins' study is interesting in that he deliberately compares two groups of students with different background characteristics, namely upper-middle class Japanese and Vietnamese refugee students in order to test the generalizability of his interdependence hypothesis (Cummins 1982). Such studies are rare. Case studies are of course also used to develop typologies of various kinds, (See Mackey 1970). There is also replication of case studies, such as the Culver City replication (Campbell 1972) of the St. Lambert study (Lambert and Tucker 1972).

Frequent variables in the experimental design type research are sex, age, ethnic identity, sense of self, vocational choice, school grades and high school attendance to mention some I culled from the Swedish research, where the behavior of migrant students is compared with those of Swedish youth. Löfgren and Ouvinen-Birgerstam make the interesting observation that this research, i.e. research on migrants, "has to a high degree been characterized by fault-finding, i.e. it has been geared to look for deficiencies" (1980:102) and go on to question the result of such research on migrants' pride and Swedes' attitudes.

I believe these types of research: the search for social conditions which are predictable, theory testing, typology building, replication and the studies with experimental design (which are inherently comparative) constitute the major functions of comparison in BE. In addition, we have Constrastive Analysis in linguistics and occasionally what amounts to a contrastive analysis even if not so labeled in anthropology, such as

Freudenthal, Narrowe, and Sachs' Turkar i svensk förort (n.d.) ('Turks in Swedish suburbia'), where it is specifically the Turks' unSwedish behavior which is singled out for description, also a form of comparison.

Eisenstadt points out that the construction of types for purposes of comparative analysis poses several methodological questions. One of these is the selection of units comparison. Ekstrand (1981a) rejects the notion of a critical period in language acquisition while Scovel (1981) breaks down language into separate skills and so argues for a critical period for the acquisition of pronunciation. Their units of comparison are different. I have already mentioned that Cummins sees it necessary to break down language acquisition into two units he calls CALP and BICS or lately "context-reduced" and "context-embedded" language proficiency (1982) where the very labels for the units contain a conceptual explanation. Context-reduced language acquisition favors older learners and so sheds further light on the critical period problem.

We talk glibly about immigrants and immigrant education but who are they? This has caused considerable definitional difficulty for the Swedish statisticians (see Reinans 1980). We know that the Finns as a group have the lowest percentage of students continuing their schooling after graduation from the compulsory grundskolan but who are the Finns? Are the Tornedalfinns Finns? Are the Swedefinns Finns? The answer may seem perfectly obvious but if the question is asked in order to extend the rights to mother tongue instruction the answer is not obvious. The rationale for selection of unit of comparison and analysis lies as always with the research question, with the purpose of the research.

Another problem is the construction of indices through which the variables can be compared. We talk about multicultural education, but what is it? I don't believe that I have ever seen a multicultural education classroom, not even in my own classroom in Tangier where we represented eight different cultures[2], but until we have a generally accepted construction of indices of what constitutes multilingual education I really can't tell because I have no way of comparing that classroom with, say, my Katrineholm classroom along the variables of culture.

Comparability of variables is a problem in most research but especially in research which uses case studies for secondary data. Comparing, as is so often done, the Canadian immersion programs with the monolingual schooling in the L_2 of migrant children is a classic error of comparison, of comparing the noncomparable. The Canadian programs show us, in Leach's terms, what is humanly feasible, but it remains to spell out under what conditions. Comparison research often compares against a known standard, be it IQ scores, TOEFL scores or standardized subject matter tests. Much of the Swedish research on migrants compares the scores for immigrant youth, (employment figures, gymnasium attendance, and test scores) with the Finnish or Swedish national norms. But immigrant youth, as both Ekstrand (1981b) and Tingbjörn and Andersson (1981) point out, are primarily members of the working class while the scores are normed on the entire population so that such comparison will give a skewed result. Sometimes faulty comparison is a result of conceptual confusion, such as

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[2]Arab, Berber, English, Gibraltarian, Indian, Spanish, Swedish, US.

comparing foreign language learning with second language learning,[3] sometimes it is just a lack of facts such as comparing a Swedish grade 9 with a US grade 9 in matters of age (Swedes begin school at age 7). I know of no formula for achieving comparability except constant vigilance of carefulness and thoughtfulness.

Comparability is closely tied to the problem of sampling. In her review of the literature on IQ and bilingualism, Darcy (1963) found a negative correlation in many studies. What she does not point out is the faulty sampling procedures of those studies: most of the subjects were recent immigrants, members of the working class, and apparently in many cases not proficient in English. In cross-national studies, sampling often involves a compromise between representativeness of group or country and equivalence across countries. Osgood et al. (1975) give the example of Indian college students being more highly selected (less representative) than an equivalent sample of Dutch college students. In the Lambert and Klineberg study Children's Views of Foreign People: A Cross-National Study (1967), they wanted to compare children's images of themselves and foreigners as a function of their nationality and age:

> Therefore they wanted both equivalence, in the sense of age levels, sex ratios, intelligence and the like, and representativeness, in the sense of socio-economic level and the like so that differences could not be attributed to education of parents, for example. This required a compromise, which in their case appeared as a form of stratified sampling within otherwise equivalent groups. (Osgood et al. (1975:20)

Osgood's point is the nature of the compromise depends on the purpose of the research.

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[3]Unfortunately Swedish for immigrants has become known as Svenska som främmande språk, 'Swedish as a foreign language' which is not helpful.

Measurement presents a number of problems. One is at the heart of all research, that of finding empirical proxies to measure for conceptual variables, the problem of operationalization. I have argued elsewhere (1980) for the need to evaluate the BE programs with indicators like school drop out rates and employment figures in addition to standardized test scores and won't repeat that argument here. Basically that is a problem in goal perception. Mostly in comparison research, I should think, the operationalization problem is one of validity of the instruments. In many of the Darcy studies, the IQ tests really functioned as language proficiency tests and it was not IQ one compared but knowledge of English. Working with case studies, one needs to pay attention to the instruments used. Time of treatment fits in marginally here. I remember one dissertation which attempted to evaluate a bilingual education program after eight hours of treatment. That is just plain silly. The Rock Point study (Rosier & Holm 1980) needed six years to show a positive result.

Sometimes the problem of operationalization has to do with finding indicators for concepts which are unique to research on bilingualism. The rationale most often cited for the Swedish policies on mother tongue teaching is that of halvspråkighet, semilingualism or more correctly double semilingualism, a notion popularized by Hansegård in 1968 which claims that bilingual children may learn neither of their two languages well. The press abounds with reference to semilingualism, the Finnish associations claim it as a rationale in their demand for monolingual Finnish university in Sweden, and university students essays repeatedly make comments that "researchers are agreed that semilingualism. . ." The fact of the matter is that semilingualism as a conceptual variable has never been operationalized to anyone's satisfaction so researchers

cannot very well agree on something that may not exist. Actually, they don't agree: Loman's work (1974) is noticeable in this context. The counter argument[4] leads into the quantitative/qualitative data discussion. Finding an empirical proxy for semilingualism will trivialize the concept, goes the claim. If it is a linguistic phenomenon, it ought to be observable, say the linguists. (See e.g. Hyltenstam & Stroud 1982) At present, there is a stand still. But as a major problem in Swedish research, operationalization of halvspråkighet remains. Before it becomes the major rationale for Swedish language policies, the existence of such a language phenomenon needs to be established, measured and described or the notion should be dismissed.

Qualitative data has other problems in comparison research. Cross-cultural anthropologists worry a lot about the generalizability of their data, mostly for reasons of validity. Would another anthropologist with other training and maybe another worldview have seen and described the same things, reached the same interpretations? In the immigrant research one will occasionally find statements, comments, poems by the immigrants themselves which is a form of operationalizing attitudes and feelings. When such statements are representative (i.e. reliable) of the group, they make excellent data, but they are of course esepcially vulnerable to researcher bias in their selection. I don't, for instance, expect to see Richard Rodriguez (1982), who writes movingly and beautifully about his

[4]Scientific concepts which have become political notions still tend to be countered by arguments couched within the realm of scientific language.

upbringing and goes on record against bilingual education, cited in Chicano research.

Another measurement problem is that of testing. There exists an entire literature on minority testing and I won't go into it here except to mention three issues in passing. First of all, there is the meaning of testing which may vary from culture to culture. Cole et al. (1971) have documented this in detail in their work with Kpelles. Donald Erickson (1969) who tested the Navajo children in the Rough Rock study wrote me in answer to a question about the testing: "In observing the students while they responded to the achievement tests, I was convinced that in many cases we were were getting measurements of attitudes regarding time, competition, the importance of tests, etc., much more than we were getting data on what the tests purported to tap. The typical achievement test, I fear is a rather stupid way of testing many American Indian students" (Private correspondence, June 26, 1973). This practical-technical objection is echoed in the conflict perspective-oriented attack by Edelsky et al.: (in press:14) on Cummins and Swain (1979) for, of all things, propagating a language deficit theory:

> When some people interpret a test question as a hostile demand for disclosure, while others interpret that same test question as a harmless demand for a performance, then it is inaccurate to claim that the two groups experienced the same standardized situation and task. The overall consequence of reliance on data from such measures and settings is to make "the hegemony of the successful look legitimate because of their superior performance on school tests" (Orasanu et al.: 1977:61).

Edelsky et al.'s major objection to Swain and Cummins is that they don't like standardized test scores and measurement research, i.e. they deny the validity of such operationalization. The neo-marxist interpretation apart,

testing does remain a problem in cross-cultural research.

There is also the problem of translation and standardization of testing instruments. Osgood et al. (1975:15ff) discuss the problems of translation, basically a question of choosing exact equivalent or culturally corresponding items, i.e. Christian Sunday corresponds with Muslim Friday. They go on to point out since the purpose of standardization is to render data from diverse samples comparable, this may well mean in cross-cultural research the deliberate choice of different instructions and even procedures. Trial and error with native informants and careful pretesting is an obvious strategy.

So far the discussion has concerned methodological problems. But as Eisenstadt points out, the construction of problems for comparative analysis also engenders theoretical and analytical problems. Types constructed out of variables imply some assumption about the importance of such variables, such as for example Cummins' CALP and BICS.

> Such analytical problems tend to become even more important in attempts to "explain" varied types of institutions, organizations, or pattern of behavior in terms of some broader conditions. In most comparative analysis such explanation aims. . .to elucidate the conditions under which such varied societal types emerge and continue to exist and function. . .(Eisenstadt 1969:425)

Let me illustrate. In order to understand the behavior of immigrants, including their language behavior, we have used the constructs of ethnicity, ethnic groups and ethnic boundaries (Barth 1969, Schermerhorn 1970). But it may well be that ethnicity is not the best choice of construct to explain the situation of the Finns in Sweden compared with that of the other immigrants. I am at this point tentatively considering the situation of the Finns as an extension of geographic nationalism rather than one

of ethnic boundary maintenance, a construct the serves well for the Turks and the Assyrians. The choice between nationalism and ethnicity then is an analytical problem and arguing that out will constitute the topic for a future paper, but briefly, that construct with its covariation of variables which most simply can account for the most data is usually considered the better choice. To the degree such a choice will carry explanatory power, it becomes a theoretical problem.

Finally I must mention the kind of problems that come under the heading of ethical problems in comparative research. They have been extensively discussed in the literature (see e.g. Brislin et al. 1975) and I mostly want just to acknowledge their existence. TESOL has adopted a set of ethical guidelines for research in English as a Second Language and I think all academics should discuss these matters with their students. The fault-finding type of research that Löfgren and Ovrinen-Birgerstam mention is an ethical problem. Research on immigrants easily enough becomes a form of internal colonization, and that is also an ethical problem. Most of all maybe, what do the immigrants get out of all this research, not in vague terms of future policies but right now? It is a glib and frequent question but difficult to answer.

Sound generalization and prediction depend ultimately on good judgment, which in turn depends on "an understanding of the various forces which underlie the process. Gaining such an understanding is more a matter of saturation in the situation than of scientific technique" (Simon 1969:357). My hope is that this discussion will help contribute to our good judgment in understanding the problems in the comparative analysis of BE so that we safely and accurately generalize from the appropriate data.

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